

**Date:** 10 Jul 2023  
**To:** "Stefanie Lane" stefanielane@utexas.edu;stefanie.lane@ubc.ca  
**cc:** "Nancy A. Shackelford" nshack@uvic.ca, "Gary E. Bradfield" garyb@mail.ubc.ca, "Madlen Denoth" mdenoth@gmx.net, "Tara G. Martin" tara.martin@ubc.ca  
**From:** "WETLANDS" claire.argote@springer.com  
**Subject:** Major Revisions requested WELA-D-23-00028

Dear Ms. Lane,

I have received the reports from our advisors on your manuscript, WELA-D-23-00028 "Plant community compositional stability over 40 years in a Fraser River Estuary tidal freshwater marsh", which you submitted to Wetlands, and have decided that your manuscript can be reconsidered for publication should you be prepared to incorporate major revisions. When preparing your revised manuscript, you are asked to carefully consider the reviewer comments which can be found below, and submit a list of responses to the comments. You are kindly requested to also check the website for possible reviewer attachment(s).

In order to submit your revised manuscript, please access our website.

Your username is: \*\*\*\*\*

If you forgot your password, you can click the 'Send Login Details' link on the EM Login page at <https://www.editorialmanager.com/wela/>.

I look forward to receiving your revised manuscript before 24 Aug 2023.

Please make sure to submit your editable source files (i.e. Word, Tex)" in all the below revision decision letters.

With kind regards,

Marinus L. Otte, Ph.D.  
Editor in Chief

#### COMMENTS FOR THE AUTHOR:

Reviewer #2: This is a well-presented manuscript on plant-community changes in an estuary, comparing historic surveys from 1979 and 1999 with a recent 2019 survey. These kind of studies are very valuable to the scientific community and I'd encourage the journal to publish this work. The statistical analyses are adequate and the authors provide convincing arguments, that the data are indeed comparable across the studies.

My main comments are around the interpretation of the observed changes. The authors discuss estuarine channel migration and shrub encroachment as two of the main reasons why some plots could not be re-surveyed. Yet these processes do not feature in the discussion of the observed changes in the re-surveyed plots. Neither were hydrological changes in the estuary referenced, such as sea-level rise rates or discharge changes in the river. This discussion is mainly centred around non-native invasive species and should be expanded to look for further possible explanations of the observed changes over the monitoring period. A clear definition of elevational the zonation of the marsh communities at the beginning of the manuscript and the shift of these over time could help to find if sediment accretion or inundation regime changes could have contributed to the observed changes and potentially allowed non-native invasive species to come in.

The recent evidence for/against the non-native status of *Phalaris arundinacea* could be further discussed.

Specific comments:

L57: only annual? Not seasonal or even daily/tidal?

L71: 'to resist change', consider whether the aim should be to resist change or to adapt to change?

L93: 'absence of significant environmental disturbance' but what about gradual changes such as accretion (aging of the marsh), subsidence or changes in inundation regimes?

Fig. 1: add scale bar and North arrow for all maps

Fig. 2D, is this the elevational zonation of the marsh? Can this be more clearly defined?

L273: 'selected'

L 339: is it the root network that traps sediment or rather the above ground biomass?

L346: add references for the evidence of non-native status of the species

L353: the 'top-down' control statement comes a bit out of the blue here and might need further introduction

L361: what about relative Sea Level Rise?

L366: 'scouring tidal surge' could you explain please, do you mean channel migration?

Reviewer #3: In general I think the primary purpose of this paper to compare vegetation composition across three time periods spanning decades is a good one. I just feel that this paper needs significant, and not inconsequential, revision both in the general writing/readability, clarity of purpose, presentation of results, and some statistical oddities. I am going to focus comments on things I considered to be "major revisions" and omit small grammar, formatting, and style issues.

Line 92: Hypothesis 1: There is an assumption being made that just because there have been no sudden/large/anthropogenic disturbances that the floristic composition should remain stable. There is ample literature suggesting otherwise that is hinted at in the introduction. If the intent was to setup a null hypothesis and then look into the effects of gradual physical changes (soil, water, etc) and those effects on composition that was never reached. To this end the paper needs to establish the physical environment during each of the time periods. For example - descriptions and data of water level/depth, sea level rise, sedimentation, winter flow rates. Each of those is likely to have a huge effect on the species composition. For example there are at least a few papers from the Great Lakes region detailing the change in community type as water levels slowly change over decades. There are papers considering floristic change from WA to CA as sea level rises and retreat is not possible creating change. In general, the physical environment is crucial to an analysis of how the floristic community is responding.

Lines 101 - 155: Methods: I thought the methodology was very under described. Transects are mentioned and slight differences in sampling there but are the actual methodologies for plant cover the same between sampling periods? The sampling itself was not very clear and I didn't think Fig 2 helped in clarification. The subset figures of each vegetation type did not aid in informing the reader and in fact seem to imply that the eventual analysis would be forced to agree with the original identification of 3 vegetation types. The order of describing methodology would be better read in this order I think: statement of how different/similar methodologies were to previous ("as close a manner as the original" doesn't get at what was and was not the same --> plant ID --> transect distribution (why were they placed where they were originally, how were those chosen?) --> estimation technique (how was visual estimation calibrated among researchers? was any line intercept used?) --> quadrat placement (current text needs clarification) --> problems, exceptions, differences etc.

There should also be a spatial analysis component here. In the methods it is stated that certain transects could not be reached because of vegetation. Presumably vegetation conditions that were not there in prior years. This implies some areas of the estuary are changing more rapidly or aggressively than others. A figure showing which transects and which portions of changed in composition.

Lines 156 - 181: Analyses:

Cluster Analysis - It looks like the break in the dendrogram was chosen specifically to produce the 3 categories originally found in 1979. The break point is not uniform across years and there is no justification given for how it was made. For example if you put a break at 35 you would have the 3 types in 1979 but only 2 in subsequent years. A deeper look into the finer vegetation clusters is warranted. The results presented show a clear change in community composition and probably spatial location so I think forcing the three community type idea is not needed and hampers to usefulness of the analysis.

Indicator Species Analysis - The proportion of importance should be shown. p-value can be indicated by using bold, italic, or something but is misleading. You can have significant species that occur at very low levels and those are usually not what we are looking at when we look at indicator species - generally we are looking for the obviously dominant ones. Or if low cover species are very important they usually have a key correlation to some sort of environmental component. Table 2 begs the question of which of these are the actual dominants. In the "Fescue" type in Table 2 we see *S. arundinaceus* go from spot one to spot 2 - to me this says it became less dominant - should the vegetation type name have changed then? Again, I feel that the original three types are being unnecessarily carried forward. In the "bogbean" type in 1999 Grass (unidentified) is the third species for what I think it probably one of the most dominant species in the type it should get a better level of identification. I get that this was not your dataset, but it should then be explained in the text.

Lines: 203-280: Results: The results should be greatly expanded upon. First, there should be a discussion of the physical environment and change over time. Then there should be an attempt at correlating the changes observed to spatial and physical components. The presented results are fine but don't do enough to consider what has changed and to lay the groundwork to discuss potential "why" in the discussion. The functional group bit begins to get at this idea but does not go far enough. Since this is an estuary, it would be important to consider if you are losing/gaining salt tolerant species, wetland species, upland species, aquatic species etc. And where the change is occurring.

Table call outs - This may be a journal/editor decision but there is extensive use of calling out tables in the supplemental material, more so than the tables included in the draft. If tables are important enough to be cited as results they should be included in the paper.

Lines 280-405: Discussion: It is frustrating to see mention of the abiotic components of the system mentioned here almost as an unattainable part of the process when it is not. Including this in some manner is crucial to improving the paper which will strengthen the results and discussion.

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